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REACTIONS OF INFUSORIA TO CHEMICALS: A CRITICISM.

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IN the January number of the *American Journal of Physiology*, Garrey[1] gives the results of extended experiments on the effects of chemicals in causing motor reactions in the flagellate Chilomonas. From the chemical side the paper is a valuable one, as being the most thorough study yet made of the specific effects of chemicals in causing reactions in unicellular organisms, and in treating the subject from the standpoint of modern physical chemistry. On the other hand, that portion of the paper which treats of the part played in the reactions by the organisms seems to me much less successful, and it is my purpose to point out certain criticisms on this part of the work.

Garrey found that the motor reactions of Chilomonas are set strongly in operation by certain chemicals, "very swift shooting movements being induced," so that the organisms, as a result of these movements, soon leave the sphere of influence of the substance in question. This effect is termed by Garrey "chemokinesis"; it is analogous to that which I have called "negative chemotaxis" in Paramecium. The chief portion of the paper is devoted to determining the relative strength of various chemicals in causing this reaction, and the exact factors in the solutions that produce the effect; interesting and valuable results are here brought out.

In certain organic acids (acetic, butyric, lactic) and their salts Garrey found the flagellates to form aggregations, as Paramecium does in solutions of CO_2 and acids of all sorts;[2]

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[1] Garrey, Walter E. The Effects of Ions upon the Aggregation of Flagellated Infusoria. *Amer. Journ. of Phys.*, vol. iii, pp. 291–315.

[2] Owing to the failure of his attempts to repeat my experiments with solutions of CO_2 and inorganic acids, Garrey denies my results with Paramecia. His failure was unquestionably due to neglect to fulfill the necessary experimental conditions. I have never known the experiments to fail when properly carried out.

these aggregations, he says, are due to "true chemotropism." In that portion of the work which deals with the way in which these aggregations are formed and with the part played by the organism, it seems to me that Garrey neglects certain facts that are absolutely fatal to the view of these matters which he puts forth. He did not determine the mechanism of the reactions of Chilomonas, — the exact way in which the organism moves when stimulated, — and this seems to me the first requisite for an understanding of how the reactions are brought about. Garrey's point of departure is his definition of *tropism* (which he says is the same as *taxis*).

Taking heliotropism as a type, he says: "In heliotropism the organism is so oriented that its axis or plane of symmetry coincides with the direction of the rays, and symmetrical points on the surface of the body are struck by the light rays at the same angle." He then quotes with approval, for the case of chemotropism, Professor Loeb's generalization: "The essence of chemotropic orientation would then consist in the animals placing themselves in such a position that symmetrical points on the surface of the body are cut by the diffusion lines at the same angle." The movement of Chilomonas into drops of organic acid, Garrey says, is "true chemotropism," and he states that the organisms move toward the center of diffusion "as if orienting themselves to radially disposed lines; lines which may represent the paths of diffusing molecules or ions." From this one might infer that Chilomonas actually does so orient itself "that symmetrical points on the surface of the body are cut by the diffusion lines at the same angle," and then swims forward in this position, as required by the generalization; Garrey says nothing to the contrary. Yet the animal does nothing of the sort. The very elementary fact seems to have escaped Garrey that Chilomonas, as is well known, is *unsymmetrical*, so that it is impossible for it to orient itself in this manner; there *are* no such symmetrical points to be thus cut. (In Chilomonas the so-called "upper" or larger lip lies at the dorso-dextral angle of the anterior end, as will be seen by consulting Fig. 9 *b* of Pl. XLV of Bütschli's *Protozoa*, so that the animal is unsymmetrical both dorso-ventrally and dextrosinistrally.) Furthermore, observation of its movements shows that it makes no attempt to do the impossible; instead of retaining any such position as the generalization calls for, it swims in spirals, a certain side of the animal being always toward the inside of the spiral; as Bütschli says, they "describe rather narrow circles" (Bütschli, *Flagellata*, p. 853). These facts, of the lack of symmetry of the organism, and its swimming in spirals, might be thought to be facts of capital importance for a decision as to whether it falls under Professor Loeb's generalization or not; they are nowhere mentioned by Garrey. It is well known that the Infusoria are prevailingly unsymmetrical, so that it is quite impossible for most of them to fall within this generalization. An attempt to apply it to one of the Hypotricha, for example (see figure on p. 262), where not only the form of the body but the structure and distribution of the locomotor organs are strikingly unsymmetrical, would bring out the absurdity of the attempt, and observation of the actual movements of the organisms, as detailed in the Fifth of my Studies,[1] only accentuates the impossibility. It may be regarded as axiomatic that a principle which requires the symmetry of organisms cannot be applied to organisms that are unsymmetrical. On this account I have not previously considered it worth while, in my Studies on Reactions, etc., to show the relation of Professor Loeb's well-known generalization to the movements of the prevailingly unsymmetrical Infusoria. The generalization was evidently made from a study of bilaterally symmetrical animals, and may be of the greatest value for an interpretation of their activities; it is obviously inapplicable to unsymmetrical organisms.

I have described the mechanism of the motor reactions of Chilomonas, on pp. 231–234 of the same number of the *American Journal of Physiology*, as that in which Garrey's paper appeared. Later study of their reactions in the production of the aggregations described by Garrey has convinced me that these aggregations are produced through the mechanism of the described motor reaction, in a manner exactly analogous to the production of the similar aggregations of Paramecium in weak acids, described on pp. 314, 315, of the Second of my Studies.[1] This possibility is not discussed by Garrey, apparently because of a preconceived view that chemotropism must take place according to Professor Loeb's generalization above quoted. He says: "Jennings's motor reaction cannot account for orientation," and, therefore, that it has nothing to do with tropisms. This unsupported sweeping denial seems a peculiar way of meeting such a detailed account of the production of orientation through the motor reaction as I have given for the thermotaxis of Paramecium on pp. 334–336 of the Second of my Studies. The general proposition that a motor reaction cannot cause orientation seems still more remarkable. Consider an organism, as in the figure, lying obliquely to the line of action of the incoming stimulus, which is indicated by the four straight arrows. Its position is at first *a–a*. After the stimulus has acted for a time, the organism is found to be oriented as shown at *b–b*, with its longitudinal axis in the direction of the lines of force. Now, how can this orientation possibly take place *except* through a motor reaction? The animal has certainly moved under the influence of the stimulus from the position *a–a* to the position *b–b*, and such a movement under the influence of a stimulus is what is commonly known as a motor reaction.[2]

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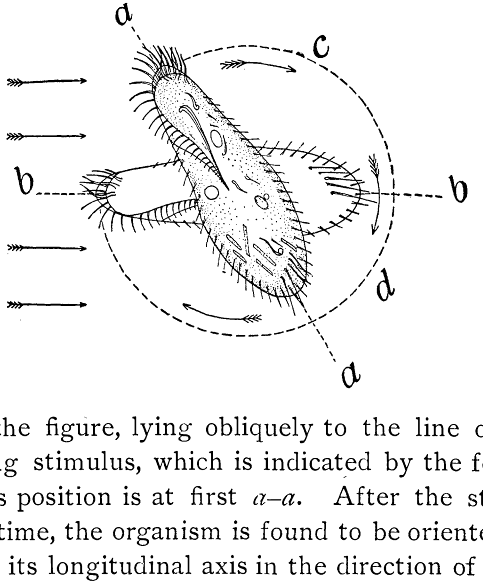
[1] *Amer. Journ. of Phys.*, vol. iii, p. 249.



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[1] *Amer. Journ. of Phys.*, vol. ii, pp. 311–341.

[2] Garrey's attempt to attribute to me the absurd idea that the principle of a motor reaction is new (*loc. cit.*, p. 313, note), is perhaps unworthy of mention. The comparison of the motor reaction of Paramecium with that of a muscle,

The essential feature of the characteristic motor reaction of these organisms, as I have described it, is that they turn, when they do turn, toward a structurally defined side, whatever the line of action of the stimulus. For example, the organism in the figure (one of the Hypotricha) turns to the right, in the direction indicated by the curve *c–d.* But evidently the organism may come into the position *b–b* as easily by turning to the right as by turning to the left; in the former case it must simply keep turning somewhat longer. As a matter of fact, orientation to most stimuli occurs in the Hypotricha in exactly the manner illustrated — by turning to the right. This is true for chemical, osmotic, mechanical, and thermal stimuli; whether for electric stimuli and light has not been shown.

The precision of the orientation, and whether a large number of organisms will be oriented in the same way, depends upon the question as to what influences produce the motor reaction. Suppose that in the figure the stimulating agent is something that acts steadily in lines coming in the direction of the straight arrows. Now, I have shown in the Fifth of my Studies that many of these organisms are much more sensitive at the anterior end than elsewhere, so that a stimulus at the anterior end produces a different effect from a stimulus elsewhere on the body. When, therefore, the organism is so oriented that the lines of force impinge directly on the anterior end (position *b–b*), it will, of course, be differently affected as compared with a position in which the sensitive anterior end is wholly or partly protected by the remainder of the body. Thus different reactions may be caused in the two cases; in one position the motor reflex may be caused, in the other not. If, for example, the motor reflex is caused when the lines of force impinge on even a small part of the anterior end, then the organism will not cease giving the reaction until it is oriented with anterior end directed away from the side of incoming

which Garrey makes for my information, I had already developed at length to show their identity in character, in two papers (*Amer. Journ. of Phys.*, vol. ii, p. 339; *Amer. Journ. of Psychol.*, vol. x, p. 513). I had counted it a chief result of my work that it had reduced the supposed attractions and repulsions of these organisms to a motor reflex "of the same order as the motor reflexes of higher animals," as I have expressed it elsewhere.

force. If a large number of such organisms are in the field of the force, they will soon all show a common orientation, and when they move they will all move in the same direction. *All that is necessary for orientation to take place in this way is that the organism shall be differently affected when it is in one position from the way it is affected in another position.* It is difficult to imagine any agency that can cause orientation in which this condition is not fulfilled.

When the organism is once oriented with reference to a constant stimulus, of course no further reaction is necessary; it simply keeps up the normal forward motion until this motion brings it into new relations with the stimulus, when the motor reaction is again caused, resulting in a readjustment of orientation. In the hypothetical case above supposed, if the organism swims in a curve instead of a straight line, it will soon come into a position such that the lines of incoming force impinge on the sensitive anterior end; the motor reaction is thereby induced until the animal has come again into the position where the anterior end is not thus affected.

Garrey says: "All these motor reactions have nothing to do with *tropisms*, for these motor reactions are only the expression of a very sudden change in the stimuli, while the characteristic of the tropism is the stationary condition of the stimuli." He fails to consider the fact that, stationary as the stimuli may be, the organisms with which he is dealing are *not* stationary; by their own movements the organisms come into new relations with the stimuli, and these new relations may result in the production of the motor reaction, causing a readjustment of orientation.

To summarize, the chief points which I would make in criticism of Garrey's conclusions are as follows:

1. In all work upon the reactions of organisms to physical and chemical agents, it is necessary to consider the structure and normal activities of the organisms, as well as the nature of the agents acting as stimuli.

2. Orientation such that "symmetrical points on the surface of the body are cut by diffusion lines at the same angle" (or by any other lines at the same angle) is impossible in unsym-

metrical organisms, such as are a large proportion of the Infusoria.

3. Observation of the movements of the Infusoria, both the unsymmetrical and the symmetrical, shows that they do not orient themselves even approximately in the manner above expressed, but that they swim as a rule in spirals, and orientation usually takes place through a motor reflex, characterized by the fact that the organism, when it turns, turns toward a structurally defined side.

4. A sudden change in the stimulus is not necessary to produce a motor reaction, as Garrey has stated, for the organism itself, by moving, comes into new relations with a constant stimulus, and by these new relations the motor reflex may be caused, resulting in a readjustment of orientation.

5. The gatherings of Chilomonas in drops of organic acid, described by Garrey, take place, according to my observations,[1] through the mechanism of the motor reaction of Chilomonas, described in the Fifth of my Studies. Whether this is "true chemotropism" or not depends on the definition of chemotropism.

6. The orientation of Chilomonas takes place through this motor reaction, and in general it is impossible for an unoriented organism to become oriented except through a motor reaction of some sort (except where passively moved, like a dead body).

ANN ARBOR, MICH., Feb. 6, 1900.

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[1] The observations on which this statement is based are detailed in a paper by the author in the April number of the *American Journal of Physiology*.